MR. ROBERT NEWSTEAD, lecturer in economic entomology and parasitology in the Liverpool School of Tropical Medicine, has been appointed to the newly-established Dutton Memorial chair of entomology in the University of Liverpool.

DISCUSSION AND CORRESPONDENCE

VITALISM AND EXPERIMENTAL INVESTIGATION

IN connection with the recent helpful discussions of vitalism by Ritter[1] and Lovejoy,[2] one point seems worthy of further emphasis. Some men are interested in science because of its bearing on general philosophical problems; others are interested in philosophical problems because of their bearing on the way to go to work in science. Both attitudes are proper enough; but one's treatment of such a question as vitalism is largely determined by which of these attitudes he takes. The point I wish to emphasize arises from the second attitude. Has vitalism (in any of the brands set forth by Lovejoy) any bearing on the theory and practise of scientific investigation?

This is a practical question in which the experimentalist as such must be interested, even though he may pride himself on his indifference to philosophical speculation. One kind of vitalism appears to me to affect fundamentally the theory of scientific work; for this reason this kind appears of more interest than the other, if not the only kind worth distinguishing.

The man of science at work with his two hands is trying to find the determining conditions for what takes place in matter and energy, and how these conditions act. In so doing he is led to make a study of the various possible methods of work, and particularly of the various ideas and devices that are presented to him as deserving consideration in his work. Many such things come to the worker in biology from outside his own special field; particularly from physics and chemistry. Such were the theories of electric dissociation; much in the physics of colloids, and the like. The biologist is compelled to examine these to see how useful they are in his own experimental analysis; often he finds them of the greatest value, and he modifies his methods of work accordingly.

Various theories of vitalism have likewise been brought to the attention of the investigator, but as a rule he has taken little interest in these, because they seemed of such a nature as not to affect his work; they seemed merely general suggestions and reflections on the fundamental meaning of what one sees in biology, of interest primarily to the man for whom science is the handmaid of philosophy, rather than the reverse. They did not attempt to provide an instrument for actual use in experimentation, nor an idea according to which scientific practise must be altered.

This appears to be the case with the first kind of vitalism distinguished by Lovejoy; a vitalism which holds that there are new modes of action in living things, but that the new modes of action are nevertheless functions of the configuration of the matter and energy involved, so that after we have discovered how a given physical configuration acts, we can depend upon it, as we depend upon such constancy in the inorganic sciences. Such a vitalism involves no fundamental change in our methods of work; we continue to test, by fitting methods, how given configurations act, and to record the results in proper generalizations, exactly as in physics and chemistry. Biology would then, so far as scientific method is concerned, bear the same relation to physics and chemistry that any unexplored part of these sciences bears to the explored parts. The distinction between vitalistic science and physical science would have but a very mild interest for the worker with his hands; it has no pragmatic bearings.

On the other hand, the second kind of vitalism distinguished by Lovejoy makes assertions which would if true require serious consideration in actual practise; indeed, it is put forward by its advocates as supplying certain factors which require consideration on the same grounds as do electric dissociation and osmotic pressure; factors without which our experimental analysis is bound to be incomplete or wrong. Its acceptance would logically

[1] SCIENCE, March 24, 1911.

[2] SCIENCE, April 21, 1911.

produce fundamental changes in the principles of experimentation. This is the point which, to me as an experimenter, seems hardly to receive adequate consideration by Professor Lovejoy; this appears to me the reason why this particular kind of vitalism (the vitalism of Driesch) has received so much attention from investigators, though as a rule they are rather indifferent to vitalistic theory. This is the vitalism which holds that the laws of what occurs in organisms "can not even be stated in terms of the number and arrangement of the organism's physical components."[3] This statement means, if it means anything, that you can not make a statement *which will hold,* that a given arrangement of physical components will act in a certain definite way (even after you have observed how it acts). If such a statement will not hold, this can be only because the same arrangement of physical components acts sometimes in one way, sometimes in another—so that there results indeterminism so far as the physical components are concerned.[4] If vitalism of this sort is correct, then the biologist can not from a knowledge of the total physical configuration predict what will happen, even after he has observed it.

To realize the situation in which this leaves the experimenter, it is needful to consider just what his work consists in. The experimental investigator is engaged in discovering the determining causes of things. Just what we are to understand by *cause* has given rise to much discussion, often leading far away from any experimental concept.[5] Experimentally it means any preceding event or condition without which the event we are studying would not have occurred. Now it turns out in experimentation that everything has a very great number of such "causes," all standing on the same experimental footing, so that to determine "the cause" of any event, taken by itself, is a hopeless task; so taken, the meaning of *cause* becomes undefinable, unless it could be held to signify finding out everything that must have happened in order that this event may occur. Progress can be made only when we so state our problem that we need search for but one determining cause at a time. Now, *one single sufficient experimental cause can be found only for the difference between two cases,* and the actual practise of experimental investigation consists in comparing two cases and finding experimentally what determines the difference between them; discovering, that is, what preceding difference results experimentally in producing the present difference.

An example will make this clear. An organism is observed to move over a certain stretch, from *a* to *b*. What is the cause of this? The question so put opens up a vast perspective; we may go into the production of the energy which brings about the movement, with the infinite number of questions that this involves; we may study the special organs by which this organism performs its movements, and how these organs were produced; we may take up the stimuli which set the organism in motion, and those which determine its direction; the environmental conditions on which the motion depends, etc. All biological science is before us; where shall we take hold? We must make our question precise, and this can be done by considering two differing cases. This specimen now swims in a certain direction; this other (or this same one at a different time) in a different direction. What is the cause *of this difference?* A little experimentation shows that the one, only and sufficient cause is the different direction of the rays of light in the two cases. Or, again, this specimen swims in a certain direction, while

[3] Lovejoy, *l. c.*

[4] A natural result of this is to do what Driesch does and what Lovejoy seems inclined to deprecate; to assume the existence of some non-physical factor, as entelechy, to supply the missing differential determining condition. This grows out of the ordinary procedure in experimental investigation; whether it really helps the experimenter in his work we shall inquire in a moment.

[5] The ambiguities in the word cause have induced some investigators to drop it entirely, and deal only with words having no implication that is not definable in experimental terms; so Verworn in the fifth edition of his "General Physiology."

this other does not, even though the direction of the rays of light is the same. What is the cause of *this* difference? Experiment shows it to be the different temperature in the two cases. In another case the difference in motion is found to be due to difference in chemical conditions; or to difference in the amount of food taken, or the like. Many times we find that there are two or more different factors, any one of which will produce the difference in question; or that the observed difference will not result unless two or more determining factors are combined. These are only details of application; the method throughout is to take two cases differing in a certain respect; then to find the (experimental) determining cause for this difference. By continuing this process, comparing all possible degrees of difference, the causal analysis may be carried to any desired degree of minuteness—till the smallest perceivable differences are reached. The process may be continued backward, tracing step by step how the determining differentials for any given case are themselves determined, until we have as full an experimental analysis as we desire, there being no end to the process of analysis, save as practical considerations compel us to stop.[6]

The investigator may of course not always actually have the two cases present before him; he may not even think of the concrete existence of more than one of the cases, but the rationale of the process, when analyzed, is that which we have set forth.[7]

Now, the fundamental principle on which this work of the investigator is based is this: *When two cases differ in any respect, there will always be found a preceding difference to which the present difference is* (experimentally) *due.* This principle is, explicitly or implicitly, constantly present with the experimenter. If two experiments, supposedly alike, give different results, *there must be some preceding difference to account for this.* The investigator is so convinced of this that it does not occur to him to doubt it or state it or consciously raise the question at all; he merely sets to work to find what the difference is, and he may spend hours or days or years in his search. This principle is the air the experimenter breathes, the water he drinks and the food he takes. It is what makes him an experimenter. If he should become convinced that it does not hold, the logical thing for him to do is to follow the finely consistent example of the sponsor for the kind of vitalism that asserts that it does not, and drop experimentation to take up philosophy.

The question whether this principle is correct need not concern us now; what I wish to bring out is the tremendous difference in scientific investigation in two fields, in one of which this principle holds (as it is supposed to do in physics and chemistry), while in the other it does not (as in biology, according to this sort of vitalism). The investigator in the field where it does not hold would be continually in doubt as to what to do. Here are two experiments that result differently. But is it worth while to search for an experimental determining factor for this difference? Perhaps there is no such factor—for this is biology, not physics. The guiding principles are different in the two fields; while we might in physics be certain that an experimental cause could be found for the difference, in biology we can not, for in biology the same configuration may give sometimes one result, sometimes another. This is a difference in principle that would really make it worth while to separate the two sets of sciences in a fundamental way; this would give us a vitalism that had some practical consequences.

But what should be the further procedure of the biologist in view of the fact that two complexes absolutely identical in their phys-

[6] The farther work, of comparing the results of this analysis and recording them in fitting generalizations, by which the heap of facts is reduced to an ordered whole, does not concern us here.

[7] Thus, when the investigator merely asks: What determines the *direction* of this movement—the experimental question essentially is, When this specimen moves in a certain direction, while another does not, what determines the difference between the two cases? Neglect to analyze problems into this form leads to much of the inconclusive work and difference of opinion in experimental biology.

ical[8] make-up give different physical results? Shall he abandon the principle of "univocal determination," not merely from a practical experimental standpoint, but completely? Or shall he follow Driesch's example and try to save the principle by assuming that the two cases differ in something non-physical (which he may call entelechy if he likes the word)? Professor Lovejoy's suggestion that this latter is a hypostasis hardly warranted in strictly scientific procedure, would leave us absolutely without determining cause for the difference; the experimenter could but admit the failure of the principle on which his work is based, lay down his arms, and surrender. But does Driesch's assumption of a non-physical differentiation between the two cases leave the experimenter in a better situation?

Driesch's statement to save the principle of determinism in such a case is as follows: "given certain circumstances, and given a certain entelechy in a certain state of manifestation, there will always be or go on one specifically determined event and no other."[9] Thus under the conditions we have sketched, the investigator could comfort himself (if he found it a comfort) with the assertion that different entelechies were at work in the two cases; or that the same entelechy was at work in different manifestations (the latter formula would be forced upon us by the vitalistic arguments from behavior). Now, what is the difference between attributing experimental results to such non-physical determiners, and the ordinary experimental procedure of attributing them to physical determiners?

The difference lies in two points (which are perhaps fundamentally one): (1) Any physical factor has various manifestations, the conditions for each of which are discoverable and constant; it is bound up in many different ways with the rest of the conditions. Hence if the experimenter attributes a result to a certain physical factor, this is at once open to test; we may try whether its other manifestations appear as they should if it is in presence; it leads at once to farther experimentation, and the explanation must stand or fall in accordance with the results of this experimentation. On the other hand, the non-physical entelechy may give different manifestations (or none at all) under the same conditions; there is no way that we can test the affirmation that a given experimental result is due to it. A physical factor that showed itself in one unique manifestation, and might later show itself under the same conditions in a different manifestation would of course leave the experimenter as helpless as does entelechy; but such a "physical factor" is a contradiction in terms; it is because entelechy has this character that it is a non-physical factor.[10] Thus attribution of a result to entelechy closes the door to farther experimental test.

2. In experimenting in non-vitalistic fields, after we have discovered what preceding differences determine (experimentally) our given diversities, we may move a step back and discover in the same way what determined those preceding differences; and this process of carrying back the experimental analysis is without end (save from practical difficulties). On the other hand, as soon as the experimenter has attributed his observed diversities of result to different manifestations of entelechy he is at the end of his experimental rope. What determines, under the same physical conditions, the different manifestations of entelechy? The problem is not only practically, but by hypothesis, beyond the reach of experimentation.[11]

[8] I am throughout using the word *physical* in place of "physical and chemical," "physical or chemical" and "physico-chemical."

[9] "The Science and Philosophy of the Organism," II., 153–154.

[10] If, as some have suggested, entelechy is to be considered merely a name for a factor whose dependence on the rest of the conditions and whose uniformity of action is not yet known, we should of course by this assumption drop our vitalistic theory; it is by vitalistic hypothesis that entelechy has the peculiarities mentioned above.

[11] "Organic systems are governed by entelechy, and therefore contain all possible future perceptible diversities in an *imperceptible* form," Driesch, "Science and Philosophy of the Organism," II., 198.

Thus the bringing in of entelechy as a determiner is at most a pad to soften the experimenter's fall; a way of distracting his attention for a moment from the dolorous fact that his method of work has failed. It merely puts off for one single step the admission that the principle on which experimental investigation is based breaks down when applied to biology. Physical science and vitalistic science are then distinguished by a fundamental difference in the principles of investigation, of the highest practical consequence.

Some attempts are made to console the biological experimenter for this difficulty; to make light of the difference in investigation in the two fields. Professor Lovejoy comforts us by saying that, in the case of the developmental processes on which Driesch partly bases his argument, you could "if you go back to an early enough stage in the given sequence of processes" find "perfectly definite, perceptible and experimentally ascertainable constant antecedents" for the observed procedure; this in view of the fact that men do not gather figs of thistles, nor whales of sea urchin's eggs; to get a given type of adult you must at least have the egg of that type. Thus only that immense field of developmental processes which lies between the egg and the adult would be exempted from experimental determinism! This might yield some solace to those whose life work does not lie in this field, were it not that Lovejoy quite leaves out of account Driesch's arguments and conclusions for the other fields of animal activities, particularly for behavior. In behavior, according to Driesch's vitalism, what the animal does depends as much on the non-physical entelechy as it does in development, and yet there is no single type toward which each act tends.[12]

Again, some have assured the writer that we may accept this kind of vitalism and still go ahead with our work just as if experimental determinism still held; that in fact cases where it doesn't hold probably occur only under rare and recondite conditions, which we may never meet. This vitalism, reserved like the religion of some individuals for Sunday consumption only, receives no encouragement from any close examination of vitalistic theory. Taking as an example Driesch's working out, we find that we may expect the vitalistic factor to show its action continually in all sorts of work with living things. According to Driesch the precise work of the vitalistic factor is to "suspend for as long a period as it wants any one of all the reactions that are possible with such compounds as are present, and which would happen without entelechy. And entelechy may regulate this suspending of reactions now in one direction and now in the other, suspending and permitting possible becoming whenever required for its purpose. . . . This faculty of a temporary suspension of inorganic becoming is to be regarded as the essential ontological characteristic of entelechy."[13] That is, when there are in juxtaposition a number of substances which, according to purely chemical laws, would interact, giving certain results, entelechy may (or may not) interfere, preventing the union of certain of these, until the resulting products are determined by those that have been allowed to interact. Thus from the same mixture of chemicals we shall get sometimes one product, sometimes another (depending on the purposes of entelechy); the variety of results thus obtainable from a given complex is of course very great. Now, all living things are complexes of great numbers of

[12] Thus Professor Lovejoy can not be followed when he states that "All that Driesch maintains is that such a [morphogenetic] process once started continues toward its normal consummation even if, after the start, some of the usual machinery instrumental to the consummation is lost and the rest has to redistribute and redifferentiate itself in order to keep things moving in the customary manner." This is only one of the observed facts on which Driesch bases his vitalistic theory; he has published an entire book on vitalism in behavior, and a large proportion of "The Science and Philosophy of the Organism" is devoted to the same subject. One gets a very inadequate idea of the real nature of his theory by supposing it limited to morphogenesis; his conclusions reach far beyond this.

[13] "The Science and Philosophy of the Organism," II., 180.

chemicals, so that the condition under which entelechy comes into play is always realized. We may therefore expect its action at any step in our work; we must be prepared at all times to find the same physical configuration giving rise now to one result, now to another; we can have no confidence that when two experiments give different results, it will be possible to find an experimental cause for this difference.

Doubtless there are investigators who can persuade themselves that they really believe this sort of thing, and yet who can continue hopefully their hopeless task of trying to discover experimentally the conditions that determine what happens—just as there are persons who assert that they believe certain orthodox religious doctrines and yet live cheerfully the life of the worldly. But for one who takes his experimental work seriously and who has use for theories only as *theories of practise,* the acceptance of such a doctrine can not fail to profoundly change his work and his attitude toward his work.[14] It takes away the guiding principle on which every step of his work is based.

Thus a doctrine which holds to consistent physical determinism in the inorganic sciences and rejects it for biology makes a tremendous difference in principle between the two fields; a difference big with practical results. I believe that to most working investigators of biology the question of vitalism means the question whether there is such a difference, and it appears unfortunate that this question should be obscured by confusing it with the (for the working investigator) relatively inconsequential question as to whether anything happens in living things that doesn't happen in those not alive.

H. S. JENNINGS

JOHNS HOPKINS UNIVERSITY,
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[14] Of course there would still be work for the biologist. Descriptive and observational work would be little affected. The biologist could substitute "entelechy" for "god" or "providence" or "nature" in the pious expositions of the naturalists of two generations ago, and devote himself to showing the wonderful and unfathomable ways of entelechy. If of an incurably analytic turn of mind he could even examine the limitations which the physical conditions place upon entelechy: and perhaps make a catalogue and classification of the various results produced by entelechy from a given physical configuration. It is the principles, methods and objects of experimentation that would be changed.

THE APPLICATION OF THE METHOD OF LEAST SQUARES

TO THE EDITOR OF SCIENCE: It would, I think, be interesting and valuable to have a consensus of opinion from both astronomers and physicists as to the limits within which the application of the method of least squares is permissible. This method is used widely by astronomers and but rarely by physicists. Moreover, I believe most physicists would hesitate to push the application of the method as far as is commonly done by astronomers.

To take a concrete case: During the discussion that followed the Saturday afternoon symposium at the recent general meeting of the American Philosophical Society, one point under discussion was whether or not the principle of relativity requires the abandonment of the concept of the ether. The writer mentioned as an *experimentum crucis* the possibility of detecting an ether-wind by measuring the speed of light in a single direction and over a path which for its greater part lay remote from the surface of the earth, thus avoiding a limitation of the Michelson-Morley experiment. It was suggested that if the measurement of the speed of light by Römer's method could be carried out with sufficient accuracy, and at two such times that the light would have to travel with and against the proper motion of the solar system, such an ether-wind might be observed. It was pointed out that the difference of time to be expected would be of the order of one fifteenth of a second. Some doubt was expressed as to whether this accuracy was yet attainable in a difficult measurement of this nature.

To this Professor Pickering replied that a large mass of such data was already in the possession of the Harvard Observatory, and had been discussed and reduced with this very